

Interactive comment on “Application of FLAI R model for early warning system in Chibo Pashyor, Kalimpong, India for rainfall-induced landslides” by Abhirup Dikshit and Neelima Satyam

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The authors appreciate the comments and suggestions for the improvement of the paper provided by anonymous referee. The detailed answers to specific comments follow below.

1. Major revisions are requested before publication, and I suggest to consider the manuscript as a technical paper and not as a research paper, because a well-known (in literature) model is applied for a selected case study.

The author accept that a well-known model is applied but disagree about the

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manuscript to be considered as a technical paper as this is a first attempt to use this model for Indian Himalayan scenario and this would encourage further research on developing early-warning landslide systems for Indian Himalayan case.

2. Introduction part is very poor in terms of references. Authors could consider many references presented in De Luca and Versace (2017), DOI 10.1007/s10346-016-0768-5, or in Guzzetti et al (2007) - Rainfall thresholds for the initiation of landslides in Central and Southern Europe - *Meteorology and Atmospheric Physics* 98(3):239-267, in order to cite many examples or schemes like ID or with antecedent precipitation.

Thank you for the careful reading and constructive comment. We have revised the text accordingly. The revised text has been marked in red.

3. For the selected case study, it should be highlighted which is the advantage of using FLAIIR model with respect to other most adopted models, like ID (in terms of number of True Positives, False Negatives, False Positives).

Thank you for the careful reading and constructive comment. We have revised the text and added in section 5.3 of the manuscript.

4. Page 10, Fig. 6: the linear transfer function is wrong. A transfer function is a filter, and its mathematical definition requires that the area of this function with the horizontal axis is equal to 1 (and this property is not respected in the figure). Consequently, also Figure 6e is wrong (the values on the vertical axis are unrealistic)

Comment accepted and modified accordingly.

5. Page 12: for the mixture of exponential functions, parameters values are the same of Sirangelo and Braca (2004), used for Sarno (Italy). Did Authors calibrate the model for their study area, or they assume the same parameters of Sarno without calibration for their case study? (in this second case I do not agree with authors). Moreover, values of mobility function are very low for the adopted parameters set.

Thank you for careful reading and constructive comment. Necessary changes have

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been made. Section 5.1 and section 5.2

6. References section should be significantly adjusted.

Comment accepted and modified accordingly.

Please also note the supplement to this comment:

<https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-295/nhess-2017-295-SC1-supplement.pdf>

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2017-295>, 2017.

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